EXPERIMENTS AND OBSERVATIONS RELATING TO THE ANALYSIS OF ATMOSPHERICAL AIR;

ALSO,

FARTHER EXPERIMENTS

RELATING TO THE

GENERATION OF AIR FROM WATER.

READ BEFORE THE AMERICAN PHILOSOPHICAL SOCIETY, FEB. 5 AND 19, 1796; AND PRINTED IN THEIR TRANSACTIONS.

TO WHICH ARE ADDED,

CONSIDERATIONS ON THE

DOCTRINE OF PHLOGISTON,

AND THE

DECOMPOSITION OF WATER.

ADDRESSED TO

MESSRS. BERTHOLLET, &c.

By JOSEPH PRIESTLEY, L. L. D. F. R. S. &c. &c.

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1796.

To Meſſrs. Berthollet, De la Place, Monge, Morveau, Fourcroy, and Haſſenfratz,

The ſurviving Anſwerers of Mr. Kirwan.

GENTLEMEN,

HAVING drawn up a ſhort defence of the doctrine of *phlogiſton*, I take the liberty of inſcribing it to *you*, as the principal advocates for the Antiphlogiſtic theory. My view in this is to draw your attention once more to the ſubject, and I requeſt the favour of an anſwer to my objections. I hope I am not wanting in a proper deference to the opinion of men ſo juſtly eminent as yourſelves and your friends in France, and alſo that of great numbers in England, and wherever chemiſtry is known, who have adopted your hypotheſis. But you will agree with me, that no man ought to ſurrender his own judgment to any mere *authority*, however reſpectable. Otherwiſe, your own ſyſtem would never have been advanced.

As you would not, I am perſuaded, have your reign to reſemble that of *Robeſpierre*, few as we are who remain diſaffected, we hope you had rather gain us by perſuaſion, than ſilence us by power. And though we are all apt to flatter

ourſelves,

ourſelves, we hope we are willing to be influenced by the former, as we are inflexible to the latter. If you gain as much by your anſwer to me, as you did by that to Mr. Kirwan, your power will be univerſally eſtabliſhed, and there will be no *Vendée* in your dominions.

Differing as we do in this reſpect, we all agree in our wiſhes for the prevalence of *truth*, and alſo of *peace*, which is wanted as much for the intereſt of philoſophy, as thoſe of humanity.

I ſubſcribe myſelf,

Your fellow-citizen,

JOSEPH PRIESTLEY.

Northumberland, in America,
June 15, 1796.

EXPERIMENTS AND OBSERVATIONS

RELATING TO THE

ANALYSIS

OF

ATMOSPHERICAL AIR.

IT is an eſſential part of the antiphlogiſtic theory, that in all the caſes of what I have called the *phlogiſtication of air*, there is ſimply an abſorption of the dephlogiſticated air, or, as the advocates of that theory term it, the *oxygen* contained in it, leaving the phlogiſticated part, which they call *azote*, as it originally exiſted in the atmoſphere. Alſo, according to the principles of this ſyſtem, *azote* is a ſimple ſubſtance, at leaſt not hitherto analyzed into any other: They therefore ſuppoſe that there is a determinate proportion between the quantities of oxygen and azote in every portion of atmoſpherical air, and that all that has hitherto been done has been to ſeparate them from one another. This proportion they ſtate to be twenty-ſeven parts of

oxygen and 73 of azote, in 100 of atmoſpherical air.

But in every caſe of the diminution of atmoſpherical air in which this is the reſult, there appears to me to be ſomething emitted from the ſubſtance, which the antiphlogiſtians ſuppoſe to act by ſimple abſorption, and therefore that it is more probable that there is ſome ſubſtance, and the ſame that has been called phlogiſton, or the *principle of inflammability* (being common to all bodies capable of combuſtion, and transferable from any one of them to any other) emitted, and that this phlogiſton uniting with part of the dephlogiſticated air forms with it part of the phlogiſticated air which is found after the proceſs; and in ſome caſes there is more of this, and in others leſs. Alſo, in ſome caſes, fixed air is the reſult of the union of the ſame conſtituent principles.

A mixture of iron filings and ſulphur, which, with a little water, has been commonly made uſe of to diminiſh and phlogiſticate air, and probably many other ſubſtances which produce the ſame effect, if they be continued in the air after the diminution has advanced to its *maximum*, occaſion an increaſe of the quantity, by an addition of inflammable air. This mixture I find to have the ſame effect when it is long confined in nitrous air, or in fixed air; and therefore it is probable

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that the ſame would be the caſe if it were confined in any other kind of air, or in vacuo. It, therefore, ſeems natural to infer, that the ſame principle which conſtitutes inflammable air was from the firſt exhaling from the mixture, but that it did not actually form inflammable air till there was no more dephlogiſticated air for it to unite with, and thereby form phlogiſticated air. The experiments from which this concluſion is drawn are recited in my former publications, and I have lately repeated them with particular attention, and the ſame reſult. I have alſo lately obſerved that by heating bones made black by burning without acceſs of air, in atmoſpherical air, there is, after the period of greateſt diminution, an increaſe of the quantity, and that it is then found to contain a mixture of inflammable air.

That ſomething is really emitted from the iron filings and ſulphur, when it is in a ſtate of diminiſhing air, is evident from the ſtrong and offenſive *ſmell* which at that time this mixture has. Flowers alſo, and eſpecially thoſe which have the ſtrongeſt ſmell, I have obſerved to phlogiſticate air. Moreover, the iron filings and ſulphur when nearly dry, emit a viſible denſe vapour, which appears by its ſmell to be vitriolic acid air, which I have obſerved to have the power of diminiſhing and phlogiſticating air; owing, no doubt, in part, to its imbibing the dephlogiſ-

ticated part of it, and with it forming common vitriolic acid; but at the ſame time part of its phlogiſton may unite with another part of the dephlogiſticated air, and with it form phlogiſticated air.

Iron filings and ſulphur, as well as phoſphorus, and moſt of the other ſubſtances which have been generally uſed for the purpoſe of phlogiſticating atmoſpherical air, do likewiſe imbibe the dephlogiſticated air contained in it, and thereby gain an addition of as much weight as the air has loſt. But this is not the caſe with *black bones* heated in air, which by this means become white; and as nothing in them is volatile, except that which conſtitutes their blackneſs, I thought they would be a very convenient ſubſtance with which to make theſe experiments.

Theſe bones gained no addition of weight in the proceſs, and when they are uſed, the diminution of the air is by no means ſo great as in the other caſes, though the air that is left is completely phlogiſticated. This is probably in conſequence of the fixed air (formed by the union of the dephlogiſticated air with the phlogiſton emitted from the bones) not being readily imbibed by the water, or any other ſubſtance with which it is then in contact; ſo that a better opportunity is given to the phlogiſton emitted from the bones to unite with that air in a different manner, and form phlogiſticated air, which

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is therefore after the proceſs found in a greater proportion than in the other caſes, to which alone a due attention has hitherto been given. When theſe bones are heated over lime water, there is a copious precipitation of the lime. Here I would obſerve, that the phlogiſton neceſſary to form this fixed air could only come from the bones in becoming white, as they had been calcined in as great a degree of heat as I could produce, ſo that no kind of air could have been expelled from them while excluded from acceſs of air.

Having by means of a burning lens heated 140.5 grains of well burned black bones in 23.75 ounce meaſures of air, it was reduced to 20 ounce meaſures, completely phlogiſticated, without any mixture of fixed or inflammable air in it. According to this experiment, the quantity of pure air in 100 ounce meaſures of atmoſpherical air was only 15.78 parts inſtead of 27.

Heating 267 grains of theſe bones in 30 ounce meaſures of air, it was reduced to 25.5 ounce meaſures completely phlogiſticated, which was in the proportion of 15 parts of dephlogiſticated air in 100 of atmoſpherical. In theſe experiments with bones there is ſometimes a ſmall loſs of weight, owing, I doubt not, to ſomething beſides phlogiſton being expelled from them by the intenſe heat of the lens; and during the

proceſs I could perceive a ſlight vapour riſing from them. When I managed the heat ſo that it was not more than neceſſary to whiten the bones, they neither gained nor loſt any weight; at leaſt the loſs was very inconſiderable.

I had ſimilar reſults from experiments made with ſmall poliſhed *ſteel needles*. For when they were heated ſo as only to become blue, and were not melted, they gained very little, if any, weight, and diminiſhed the air only in about the ſame proportion with the black bones.

Having by means of a burning lens heated 200 grains of the poliſhed needles in 24 ounce meaſures of air (in conſequence of which they became of a dark colour) they neither gained nor loſt any weight, and the air was reduced to 19.5 ounce meaſures, almoſt completely phlogiſticated. I heated the ſame quantity of theſe needles in 16.75 ounce meaſures of air, when it was reduced to 13.5 ounce meaſures, completely phlogiſticated without any mixture of fixed or inflammable air in it; ſo that the diminution was in the proportion of 19.4 parts in one hundred. In another experiment 24.75 ounce meaſures of air were reduced to 20.25 ounce meaſures nearly phlogiſticated. It is evident, therefore, from theſe experiments, that more phlogiſticated air is found after this proceſs, than after that with the iron filings and ſulphur.

Becauſe by heating the needles over a quantity

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of water they might attract moiſture, which cannot be expelled from them without ſome difficulty, I heated 200 grains of the ſame needles in the open air, till they had aſſumed exactly the ſame appearance with thoſe that had diminiſhed the air in the preceding experiments, and found that they had neither gained nor loſt any ſenſible weight. The ſame was the reſult of whitening a quantity of black bones in the open air. But to make this experiment with accuracy, the bones ſhould be calcined with the greateſt degree of heat, and made white with the leaſt.

In one experiment with very thin pieces of malleable iron (viz. iron turnings) 38.5 ounce meaſures of air were reduced to 31.5 meaſures, wholly phlogiſticated, which is in the proportion of the loſs of 19.5 parts in 100. I could not perceive that the iron had gained or loſt any weight; whereas, if it had imbibed the air that had diſappeared, or the water, of which, as I have ſhewn, the air principally conſiſts (as it would have done if it had been melted in the proceſs) it ought to have gained 4.2 grains.

There was not, however, the ſame certainty in the experiments with the needles, and ſtill leſs with the iron, as in thoſe with the bones. They generally gained a little weight, and diminiſhed the air more than the bones. The reaſon of this uncertainty might be, that they were

were ſometimes heated too much; and ſometimes fine ſcales were thrown from them, which were indeed ſometimes viſible when, in floating about within the veſſel, they croſſed the ſun beams, and both in the experiments with the needles and thoſe with the bones a vapour viſibly roſe from them. When the needles were heated over lime water, a thick cruſt was formed upon it; but there was not ſuch a precipitation of the lime as in the experiments with the bones.

That the phlogiſtication of nitrous acid is owing, in ſome caſes, to its *imbibing* ſomething, and not always to its *parting* with any thing, which the antiphlogiſtians maintain, is evident from its becoming phlogiſticated by imbibing nitrous air. This I have obſerved that it does with the greateſt rapidity, leaving in ſome caſes not more than one 18th part of the original quantity. M. Fourcroy ſuppoſes (*Philoſophie Chymique*, p. 76) that the converſion of the common nitrous acid into the phlogiſticated is always occaſioned by its parting with oxygen. That this is ſometimes the caſe I have demonſtrated in my experiments with heating it in long glaſs tubes; but in the preſent caſe it is not poſſible that the acid ſhould have parted with any thing, and leaſt of all with *oxygen*, ſince the ſmall reſiduum of nitrous air is pure *azote*. I ſhall here obſerve, what I did not before, that

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the abſorption of nitrous air by nitrous acid is attended with a conſiderable degree of heat.

That phlogiſticated air, or azote, is not a ſimple ſubſtance, but conſiſts of phlogiſton (or whatever is the proper element of inflammable air) and of dephlogiſticated air, is probable from ſeveral experiments that ſeem to have been overlooked by the antiphlogiſtians, ſuch as the following. A mixture of dephlogiſticated and inflammable air being kept a long time, was found by M. Metherie to contain a conſiderable portion of phlogiſticated air, as appeared by the difference of the reſiduums after exploding a part of the mixture when firſt made, and another part ſome time afterwards. I had alſo found that a mixture of dephlogiſticated and inflammable air ſuffers a conſiderable diminution in a courſe of time, though they will not wholly incorporate. But I have lately found that theſe two kinds of air unite completely by being confined ſome time together in a moiſt bladder.

Having mixed equal quantities of thoſe kinds of air, I put them into a bladder, which I left floating in a trough of water, and found, after about a fortnight, that the quantity was conſiderably diminiſhed; and examining it, I found it to be almoſt wholly phlogiſticated, though there was ſomething ſlightly inflammable in it. On this I put equal meaſures (but omitted to note the quantity) of each of the kinds of air into another

another bladder, and after about three weeks, found it reduced to 12.5 ounce meaſures, all pure phlogiſticated air, without any mixture of fixed or inflammable air that I could perceive.

I have likewiſe hit upon another method of forming phlogiſticated air by the union of dephlogiſticated and inflammable air, viz. by expoſing the latter to a ſurface of ruſted iron, which is known to become ſo by imbibing pure air. Twenty ounce meaſures of inflammable air were confined in a phial containing pieces of ruſted iron from the 18th of Auguſt to the 6th of October, when it was reduced to 9 ounce meaſures, and was but ſlightly inflammable. The iron, from being red, was then become of a very dark colour. Another quantity of inflammable air treated in the ſame manner from, I believe, the 6th of October, was in the 2d of December found to be completely phlogiſticated. In theſe experiments the iron and the air were confined by water. Afterwards, putting 7 ounce meaſures of inflammable air to pieces of ruſted iron confined by mercury, it was, in about a week, almoſt wholly abſorbed. I then filled up the veſſel again with inflammable air, and when the diminution proceeded no farther, I examined it, and found 5 ounce meaſures of air completely phlogiſticated.

Charcoal, as well as phlogiſticated air, I have no doubt, contains the element of dephlogiſti-

cated

cated air, as well as phlogiſton, ſince by its union with ſteam it takes the form of fixed air, as well as that of inflammable air, and one element in the compoſition of fixed air is dephlogiſticated air. And when I made hot charcoal imbibe inflammable air by introducing pieces of it into jars of this air confined by mercury, and afterwards expelled it by plunging the charcoal in water, that which came out of it was phlogiſticated air. Yet I think I recollect that the reſult of this experiment has ſometimes been inflammable air, the ſame that the charcoal had imbibed.

I know of no caſe of the ſimple abſorption of air, but which, like that by water, reſpects all kinds of air, though with a preference of that which is dephlogiſticated; but not ſo as to take this kind *only*, and leave all the phlogiſticated air that was mixed with it. Otherwiſe it would have been in our power to aſcertain with exactneſs the real proportion of both the kinds of air in the atmoſphere. For want of this the neareſt approximation that we can make appears to me to be by the uſe of nitrous air.

Since when two meaſures of pure nitrous air are mixed with one meaſure of pure dephlogiſticated air, they both, as nearly as poſſible, diſappear, and form nitrous acid, which is imbibed by the water in which the mixture is made, it is evident that little or no phlogiſticated air is

neceſſarily formed in this proceſs; and when it is conducted properly, there will appear to be a much greater proportion of dephlogiſticated air in the atmoſphere than has been ſuppoſed, and enough to be converted into phlogiſticated air in the proceſs above mentioned. But a conſiderable *time* is neceſſary for this purpoſe; becauſe the diminution continues much longer than has been hitherto imagined.

The difference between the degree of diminution of atmoſpherical air by a mixture of nitrous air, with, or without, *agitation*, is very conſiderable. In general, without agitation, equal meaſures of each will occupy the ſpace of 1.25 meaſures, but with agitation only 1.01; and if the computation be made from this laſt *datum*, it will give the proportion of dephlogiſticated air to be 27 parts in 100, and conſequently that of the phlogiſticated air 73. But by keeping the mixture a longer time, the diminution will proceed to about 0.6 of a meaſure, which will give 46.6 for the proportion of dephlogiſticated air, and 53.4 for that of the phlogiſticated air in the atmoſphere.

This diminution in the mixture of nitrous and atmoſpherical air, which is effected in the courſe of time, is various, depending, no doubt, on ſeveral circumſtances which I have not yet been able to aſcertain. What I have actually obſerved is as follows:

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On the 21ſt of July I mixed equal quantities of nitrous and atmoſpherical air; when, with agitation, they occupied the ſpace of 1.01. Examining the mixture at different times, I obſerved that the diminution kept advancing till ſome time before 24th of Auguſt, when it occupied the ſpace of only 0.545. Another mixture made in the ſame manner was 0.54, and another 0.65. At the ſame time I found other mixtures made without agitation, which at firſt occupied the ſpace of 1.25, were in one caſe 0.75, another 0.72, and another 0.65.

The reaſon why I apprehend the diminution goes on ſo long is, that time is requiſite for the action of the phlogiſton in the nitrous air upon the dephlogiſticated part of the atmoſpherical air, in order to the converſion of the whole of it into nitrous acid, in conſequence of this part being intimately diffuſed through the phlogiſticated part, by which it is, as it were, protected from its action, which is ſimilar to many other chemical proceſſes. It is for the ſame reaſon that the diminution is much greater with agitation than without it, as the parts diſpoſed to unite are thereby brought into better contact.

When atmoſpherical air is exploded together with inflammable air, the diminution never proceeds ſo far as when nitrous air is mixed with it; becauſe in this caſe phlogiſticated air, as well

well as nitrous acid, is formed by their union; and, as I have ſhewn, the greater is the proportion of the inflammable air employed, the greater will be the proportion of phlogiſticated air in the reſiduum. This mixture, however, will go on diminiſhing for ſome time, though not ſo far as that with the nitrous air; becauſe part of this produce being nitrous acid, as I have ſhewn in a former courſe of experiments, it will require time to be formed, as well as when the nitrous air is employed.

Having made a mixture of equal parts of inflammable and atmoſpherical air, and exploded them on the 3d of Auguſt, I obſerved that it then occupied the ſpace of 1.35 meaſures, and on the 2d of September, when I perceived that the diminution would proceed no farther, it was 1.14 which, though conſiderable, was far ſhort of the diminution produced by an equal bulk of nitrous air.

Though, in the experiments recited above with the *calcined bones*, and the *ſteel*, neither of theſe ſubſtances appeared to have loſt any weight that I was able to aſcertain, it does not follow that nothing was emitted from them. For *light* and *heat* are almoſt univerſally allowed to be *ſubſtances*, though no perſon has been able to weigh them. Beſides the quantity of the materials that I made uſe of might be too ſmall for the purpoſe. What

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is moſt important in the experiments is that, ſince the diminution of the air was effected by heating thoſe ſubſtances, and they did not *gain* any weight in the proceſs, the phlogiſtication of air is not the abſorption of any part of it by the the ſubſtance which produces that effect, as the antiphlogiſtic theory ſuppoſes.

FARTHER EXPERIMENTS

RELATING TO THE

GENERATION OF AIR FROM WATER.

IN a late publication, containing an account of ſome experiments relating to *the generation of air from water**, I mentioned three different proceſſes in which air was produced from the ſame water, without any perceivable limit.

The firſt proceſs was converting the whole of a quantity of water into ſteam, in the common method of boiling; when I found that, though the water had been boiled ever ſo long, or ever ſo often, air continued to be produced from it.

In order to obviate the objection to the water having imbibed the air from the atmoſphere, in a ſecond proceſs I put the water on which I operated into long glaſs tubes, over a column of mercury; and after producing air by keeping the upper part of the tube containing the water a long time in the form of vapour, I let out the air ſo procured under mercury, by which means the water never came into any contact with the air of the atmoſphere, and yet it continued to yield air whenever the proceſs was repeated, without any perceivable diminution, or limit.

* Read before the Philoſophical Society of London, and published ſeparately in 1793. E.

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In the third proceſs, no heat was uſed, but the water was put into a glaſs veſſel conſiſting of a large bulb, connected with a tube the full length of a barometer, a quantity of mercury ſufficient to fill the tube being put into it along with the water, and then inverted, and placed in a baſon of mercury. By this means the preſſure of the atmoſphere was removed from the water, and thus the air naturally contained in it eſcaped, and lodged on the ſurface of the water; and by inverting the veſſel again, it was thrown out into the open air. This proceſs I kept repeating with the ſame water more than a year, and yet, as in the former proceſſes, I found freſh air always produced from it, and ſeemingly in an equable manner.

It has been ſaid that, in this proceſs, the water, deprived of all air, inſtantly ſeizes upon ſome the moment that the newly extricated air is thrown out, the ſurface of the water in the tube being then, though but for a moment, expoſed to the atmoſphere. But this ſuppoſed eager attraction of air by the water would have made it to abſorb the newly produced air, if not in its rarefied ſtate on the ſurface of the water, yet when it was condenſed, on inverting the tube, during the time that it was paſſing the whole length of the tube, as readily as freſh air from the atmoſphere. Beſides, it requires a conſiderable time before the water thus deprived of all air will abſorb that

which has been produced, or extricated from it, when the veſſel is inclined, and conſequently the preſſure of the atmoſphere removed.

Alſo, in order to obviate this objection, I kept the extremity of the tube carefully covered with my finger all the time that it was inverted till the moment that the air muſt be let out, and mercury put in, ſo that it was not expoſed to the atmoſphere ſo much as the tenth of a ſecond; and yet I found repeatedly, that the air was produced as readily as when it had been expoſed to the atmoſphere (as I ſometimes purpoſely did it) ſeveral minutes.

I would farther obſerve, that in this proceſs, if the veſſel containing the water and mercury be inverted, and a vacuum appear, as it inſtantly will, in the form of a bubble, for ever ſo ſhort a time, a perceivable bubble of permanent air will be produced. I do not therefore ſee but that, by means probably of heat, air is producible from the ſame water without any limit.

In order to make any quantity of water as free from air as poſſible, *agitation* is neceſſary. But when by the frequent repetition of this proceſs the greateſt effect has been produced, and the air, or vapour, has remained long upon the water, agitation will diminiſh it, part of the newly generated air being imperfectly formed, and more readily imbibed by the water than

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that which had been a longer time in the ſtate of air. This diminution of the bulk of a bubble of air by agitation appears to be the moſt certain teſt of as perfect an extrication of air from water as can be attained. But even after this, whenever the bubble of air is let out, and the veſſel is inverted, another bubble is inſtantly formed, ſometimes indeed ſo ſmall as not to be viſible to the naked eye, but always by means of a magnifying glaſs, and this very ſmall quantity will not be abſorbed by the water till the veſſel has been laid in an inclined poſition ſome hours. If the veſſel be placed perpendicularly, the bubble will come to be of a conſiderable ſize. Still however it will not increaſe beyond a certain quantity, though it remain in that poſition ever ſo long.

I have tried every method that I could think of to deprive water of its power of producing air, but without effect. *Heat* I found of no uſe but to aſſiſt in expelling the air originally contained in it, and freezing had no more effect than heat.

When I publiſhed the pamphlet above mentioned I had not procured from water any other kind of air than ſuch as was, in a greater or leſs degree, purer than that of the atmoſphere, and therefore I imagined that this might have been the origin of all the air in the atmoſphere.

But I have ſince found that though the firſt quantity of air that is expelled from water is much purer than that of the atmoſphere, the next is leſs pure, and at laſt it is wholly phlogiſticated. This I could not diſcover while I made uſe of ſmall bulbs; but when I uſed large ones, containing from fifty to an hundred ounces of water, it was aſcertained with the greateſt certainty. From this fact it may be inferred, either that the air produced from water is not that which had been imbibed from the atmoſphere, or that, though it imbibes moſt readily that which is the pureſt, it retains with the greateſt obſtinacy that which is leaſt pure, which is analogous to other chemical affinities. If the air thus produced be really generated from the water, or rather vapour, it muſt be wholly phlogiſticated, and afterwards purified by the proceſs of vegetation; or the phlogiſticated part alone of the atmoſphere may have had that origin, and the dephlogiſticated part have come from vegetation.

I once thought that a very ſmall quantity of any of the *acids* enabled water to yield more air than it would do without them, and while I uſed only ſmall bulbs, I continued to think ſo; but when I uſed the larger veſſels above mentioned, I could not perceive any ſenſible difference in the reſults in conſequence of this circumſtance.

The quantity of air extricated from diſtilled water before the production becomes equable is about one fortieth of its bulk.

Wiſhing to leave nothing undone that I was capable of doing with reſpect to this courſe of experiments, I have, ſince the publication of the tract above mentioned, endeavoured to convert the whole of a ſmall quantity of water into air, but it has been without effect. Having provided barometer tubes with bulbs connected with them, from one inch to three inches in diameter, I firſt put into them a ſmall quantity of water, and then filling them with mercury, left them ſome time with the orifices of the tubes upwards, in order to give the water an opportunity to riſe to the top. I then inverted them, when after ſome time a very ſmall quantity of water would be viſible on the ſurface of the mercury in the tube, and the vapour ariſing from it in *vacuo* would, of courſe, be diffuſed through the whole of the bulb above it. After this, inclining the veſſel, and making it lean over a fire, that ſmall quantity of water was wholly converted into vapour, ſo as to cauſe the mercury to deſcend, and leave both the whole of the bulb, and part of the tube, filled with hot vapour, and in this ſtate I kept it ſeveral hours. After this I always found a quantity of air produced, and this I threw out,

by inverting the veſſel. Then expoſing it again to the heat, I never failed to get more air; and having done this, in ſome caſes, not leſs than twenty or thirty times, I was ſatisfied that even the ſmalleſt quantity of water will never ceaſe to yield air, and in ſeveral caſes I have by this means procured more air than the bulk of the water.

As ſome water would neceſſarily inſinuate itſelf between the mercury and the glaſs, I expoſed almoſt the whole of the tube containing the mercury to the heat; by this means converting that water into vapour, and making it aſcend to the top of the mercury; then throwing out both the water and the air produced from it again and again, I at length found nothing but air above the mercury. Still, however, the whole of the water was not converted into air. For when, by means of heat, the mercury was made to deſcend, the water which had been confined between the mercury and the glaſs made its appearance, though by the aſcent of the mercury it would again diſappear.

I have alſo found that when there was any ſenſible quantity of water above the mercury, and have expoſed it to heat day after day, the quantity of air, in this caſe as well as the preceding without heat, came to a *maximum*, and

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no repetitions of the proceſs would increaſe it. This induces me to conclude that the longeſt continuance of any quantity of water in the ſtate of vapour would not convert it into air. It may, however, be worth while, if there ſhould be an opportunity of doing it without much expence, to make the experiment.

The pureſt diſtilled water ſhould be uſed in theſe experiments. Inſtead of this, I once uſed pump water; but found that, after the production of air was advanced to its maximum, it began to yield a conſiderable quantity, at leaſt ten times more than it had done before, at the ſame time becoming a little turbid. But when it was clear, it ſtill yielded much more air than diſtilled water. Probably ſome calcareous matter diſſolved in the water was decompoſed in this proceſs, and the air contained in it had increaſed the bulk of that which had been produced by means of the water.

Having, in the manner above mentioned, found an eaſy method of expelling from a quantity of water all the air contained in it, I wiſhed to know what would be the reſult of making it imbibe different kinds, and various mixtures of air. I had before found that water deprived of its air by boiling would imbibe any kind of air, and that when this air was again expelled by heat, the quality of it was not changed; but

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I could now both expel the air more effectually, and make it imbibe any particular kind of air with more certainty and expedition. For this purpoſe, having firſt expelled the air, by removing the preſſure of the atmoſphere in the manner deſcribed above, I inclined the veſſel, laying it in a poſition nearly horizontal, with the end of the tube immerſed in a baſon of mercury; and then having introduced the air that I wiſhed it to imbibe, I gently agitated the veſſel, and the preſſure of the atmoſphere being now removed, the water would pretty ſoon ſaturate itſelf with the air. After this, the veſſel being placed upright, the air which it had imbibed was preſently diſcharged, without any application of heat.

In this method, beginning with atmoſpherical air, which conſiſts of a mixture of dephlogiſticated and phlogiſticated air, I found that water imbibes the former in preference to the latter, but not wholly unmixed with it. Having made 45 ounces of diſtilled water free from air, I put to it $2\frac{3}{4}$ ounce meaſures of atmoſpherical air, of which, by agitation, it imbibed three fourths of a meaſure, when the remaining two ounce meaſures were found to be of the ſtandard of 1.15 inſtead of 1.01 which was the ſtandard of the air before the proceſs; that is, when one meaſure of this air was

mixed

mixed with one meaſure of nitrous air, it occupied that ſpace. When the air that had been imbibed was expelled from the water, it was of the ſtandard of 0.75. Both mixed together were exactly of the ſtandard of atmoſpherical air.

I had thought that, though dephlogiſticated and inflammable air will not unite while they retain their aërial form, without a red heat, they might do ſo when they were both deprived of that form, by being combined with water, and make phlogiſticated air; I therefore made a quantity of water deprived of all air imbibe a mixture of equal quantities of thoſe two kinds of air. But when this mixed air was expelled from the water, it was fired with an exploſion, ſo that no union had been formed between them. I then made a quantity of water imbibe the two kinds of air one after the other, but there was no difference in the reſult. The air that was expelled from the water was ſtill fired with one exploſion.

But dephlogiſticated and nitrous air, which unite without heat in their aërial form, did the ſame when they were combined with water. Having expelled all its air from a large quantity of diſtilled water, I firſt made it imbibe as much as it could of nitrous air, and after that of dephlogiſticated air, and obſerved that what remained of each, not abſorbed by the water, was very little changed.

changed. Then, expelling the air from the water thus doubly impregnated, the firſt quantity procured was dephlogiſticated, though not ſo pure as before; the ſtandard of the proceſs with two equal quantities of nitrous air being 0.6, whereas before it had been 0.2. The ſtandard of the ſecond expulſion of air was 0.4. Afterwards it was 0.8, then 1.0; and thus it would, no doubt, have proceeded, till it had been wholly phlogiſticated; but no part of it had the property of nitrous air. This kind of air that had been imbibed muſt have united with as much of the dephlogiſticated air contained in the water as it could ſaturate, and thus have formed nitrous acid, which remained in the water, while the ſuperfluous dephlogiſticated air had been expelled in the proceſs.

I then firſt ſaturated the water with the dephlogiſticated air, and after that with the nitrous air, which it imbibed very readily; and expelling the air afterwards, found it to be purely nitrous, there having been more nitrous air employed at this time than was ſufficient to ſaturate the dephlogiſticated air.

Having made the preceding experiments with *water*, I wiſhed to extend them to other liquid ſubſtances, and began with *ſpirit of wine*, which I had before found to be convertible into inflammable air by a red heat, and alſo

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by the electric ſpark. I now find that ſo great a degree of heat is by no means neceſſary for this purpoſe.

If I fill one of the bulbs above mentioned with the ſpirit, and by means of a column of mercury take off the preſſure of the atmoſphere, a very great quantity of inflammable air is immediately diſcharged from it, and by a repetition of the proceſs a ſmaller quantity never fails to be produced, and as far as I ſee without limits.

If in this ſtate I expoſe the ſpirit to a degree of heat ſufficient to convert it into vapour, a very great proportion of it is preſently converted into air, and in a few minutes the quantity produced will be ten or twenty times the bulk of the liquid. This is the caſe repeatedly with the ſame ſpirit, ſo that I have no doubt but that, in time, the whole of it would be ſo converted, juſt as if it had been expoſed to a red heat in paſſing in the form of vapour though a red hot earthen tube.

Having expelled a very great quantity of air from one of the bulbs filled with ſpirit of wine, of the ſpecific gravity of 682.5, I expoſed it to the atmoſphere, after which it yielded as much as before, viz. about one third or one fourth of its bulk, all ſtrongly inflammable. I had the

ſame

ſame reſult in the ſubſequent proceſs. After another, the air was exploded like a mixture of inflammable and atmoſpherical air, and the next produce burned with a lambent flame. Being then examined, its ſpecific gravity was 692.4; ſo that it had acquired ſome weight by imbibing atmoſpherical air.

Having, in like manner, expelled air which I found to be inflammable from a quantity of *ſpirit of turpentine*, I made it imbibe atmoſpherical air, and expelling it again, found it to be not inflammable, but phlogiſticated. This fluid had alſo gained ſomething in its ſpecific gravity by the proceſs.

The only objection that, after giving much attention to the ſubject, I think can be made to the concluſion that I firſt drew from theſe experiments, viz. that air is actually produced from water, is the very ſmall quantity that is produced in proportion to the bulk of the water, after the air naturally contained in it is wholly expelled. But if it ſhall appear, after a long courſe of time, that this ſmall production of air from the ſame water is conſtant and equable, I do not ſee how the concluſion, extraordinary as it may be thought, can be diſputed. This air being wholly *phlogiſticated* is a ſufficient proof that the air ſo produced is not

abſorbed from the atmoſphere in the courſe of the proceſs. For then it would have been dephlogiſticated, or at leaſt purer than that of the atmoſphere, which water always ſeizes upon in preference to that which is impure.

THE END.

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CONSIDERATIONS

ON THE

DOCTRINE OF PHLOGISTON,

AND THE

DECOMPOSITION OF WATER.

By JOSEPH PRIESTLEY, L. L. D. F. R. S. &c. &c.

Qualem commendes etiam atque etiam aſpice. HORACE.

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CONSIDERATIONS

ON THE

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THE INTRODUCTION.

THERE have been few, if any, revolutions in ſcience ſo great, ſo ſudden, and ſo general, as the prevalence of what is now uſually termed *the new ſyſtem of chemiſtry*, or that of the *Anti-phlogiſtians*, over the doctrine of Stahl, which was at one time thought to have been the greateſt diſcovery that had ever been made in the ſcience. I remember hearing Mr. Peter Wolfe, whoſe knowledge of chemiſtry will not be queſtioned, ſay, that there had hardly been any thing that deſerved to be called a *diſcovery* ſubſequent to it. Though there had been ſome who occaſionally expreſſed doubts of the exiſtence of ſuch a principle as that of *phlogiſton*, nothing had been advanced that could have laid the foundation *of another ſyſtem* before the labours of Mr. Lavoiſier and his friends, from whom this new ſyſtem is often called that of the *French*.

This ſyſtem had hardly been publiſhed in France, before the principal philoſophers and chemiſts of England, notwithſtanding the rivalſhip which has long ſubſiſted between the two countries, eagerly adopted it. Dr. Black in Edingburgh, and as far as I hear all the Scots have declared themſelves converts, and what is more, the ſame has been done by Mr. Kirwan, who wrote a pretty large treatiſe in oppoſition to it. The Engliſh reviewers of books, I perceive, univerſally favour the new doctrine. In America alſo, I hear of nothing elſe. It is taught, I believe, in all the ſchools on this continent, and the old ſyſtem is entirely exploded. And now that Dr. Crawford is dead, I hardly know of any perſon, except my friends of the Lunar Society at Birmingham, who adhere to the doctrine of phlogiſton; and what may now be the caſe with *them*, in this age of revolutions, philoſophical as well as civil, I will not at this diſtance anſwer for.

It is no doubt *time*, and of courſe opportunity of examination, and diſcuſſion, that gives ſtability to any principles. But this new theory has not only kept its ground, but has been conſtantly and uniformly advancing in reputation, more than *ten years*, which, as the attention of ſo many perſons, the beſt judges of every thing relating to the ſubject has been unremittingly given to it, is no inconſiderable period.

period. Every year of the laſt twenty or thirty has been of more importance to ſcience, and eſpecially to chemiſtry, than any ten in the preceding century. So firmly eſtabliſhed has this new theory been conſidered, that a *new nomenclature*, entirely founded upon it, has been invented, and is now almoſt in univerſal uſe; ſo that, whether we adopt the new ſyſtem or not, we are under the neceſſity of learning the new language, if we would underſtand ſome of the moſt valuable of modern publications.

In this ſtate of things, an advocate for the old ſyſtem has but little proſpect of obtaining a patient hearing. And yet, not having ſeen ſufficient reaſon to change my opinion, and knowing that free diſcuſſion muſt always be favourable to the cauſe of truth, I wiſh to make one appeal more to the philoſophical world on the ſubject, though I have nothing materially new to advance. For I cannot help thinking that what I have obſerved in ſeveral of my publications has not been duly attended to, or well underſtood. I ſhall therefore endeavour to bring into one view what appears to me to be of the greateſt weight, avoiding all extraneous and unimportant matter; and perhaps it may be the means of bringing out ſomething more deciſive in point of *fact*, or of *argument*, than has hitherto appeared.

No perſon acquainted with my philoſophical publications can ſay that I appear to have been particularly attached to any hypotheſis, as I have frequently avowed a change of opinion, and have more than once expreſſed an inclination for the new theory, eſpecially that very important part of it *the decompoſition of water*, for which I was an advocate when I publiſhed the ſixth volume of my experiments; though farther reflection on the ſubject has led me to revert to the creed of the ſchool in which I was educated, if in this reſpect I can be ſaid to have been educated in any ſchool. However, whether this new theory ſhall appear to be well founded or not, the advancing of it will always be conſidered as having been of great importance in chemiſtry, from the attention which it has excited, and the many new experiments which it has occaſioned, owing to the juſt celebrity of its patrons and admirers.

SEC-

SECTION I.

OF THE CONSTITUTION OF METALS.

ACCORDING to the doctrine of phlogiſton, advanced by Becher and Stahl in the beginning of this century, and much ſimplified and improved ſince their time, metals, phoſphorus, ſulphur, and many other ſubſtances which are ſuppoſed to contain it, are compounds, conſiſting of this principle, and another which may be called its *baſe*. Thus each of the metals contains phlogiſton united to a peculiar calx, and ſulphur and phoſphorus conſiſt of the ſame principle and their reſpective acid, or the baſes of them. But according to the antiphlogiſtic theory, all the metals are ſimple ſubſtances, and become calces by imbibing pure air; and ſulphur and phoſphorus are alſo ſimple ſubſtances, and become the acids of vitriol and of phoſphorus by imbibing the ſame principle, called by them *oxygen*, or the principle, as it probably is, of univerſal acidity.

As a proof that metals are ſimple ſubſtances, and that they become calces merely by imbibing air, they allege the caſe of mercury, which becomes the calx called *precipitate per ſe* by expoſure to the atmoſphere in a certain degree of heat, and which becomes run-

ning mercury again by expoſure to a greater degree of heat. They therefore think it impoſſible not to conclude, that in all other caſes of calcination, as well as this, the only difference between the calx and the metal, is that the latter has parted with the air which it had imbibed.

But this is the caſe of only this particular calx of this metal, and there is another calx of the ſame metal, viz. that which remains after expoſing turbith mineral to a red heat, which cannot be completely revived by any degree of heat, but may be revived in inflammable air, which it imbibes, or when mixed with charcoal, iron-filings, or other ſubſtances ſuppoſed to contain phlogiſton. And if this calx of mercury, or (ſuppoſing it to contain ſome acid of vitriol) this ſalt, neceſſarily requires ſome addition to conſtitute it a metal, all mercury muſt contain the ſame. For though with the ſame external appearance, the ſame metal may contain different proportions of any particular principle, as phlogiſton, they muſt be denominated different ſubſtances, if ſome ſpecimens contain this element, and others be wholly deſtitute of it. All, therefore, that can be inferred from the experiment with the precipitate per ſe is, that in this particular caſe, the mercury in becoming that calx imbibed air, without parting with any, or very little of its

its phlogiſton; and if we judge by the air expelled from the calces of metals and other circumſtances, there are few, if any, of them but contain more or leſs of phlogiſton.

I would obſerve in this place, that it is aſſerted by ſome very able chemiſts, that if the precipitate per ſe be made with proper attention, it will be revived without yielding any air. This is alſo the caſe with *minium* when freſh made. But this is owing, I doubt not, to their wanting *water*, which I deem to be eſſential to the conſtitution of every kind of air; ſo that they both contain the element of dephlogiſticated air, though, for want of water, it is not able to aſſume that form.

That mercury may have the ſame external appearance, and all its eſſential properties, and yet contain different proportions of ſomething that enters into it, is evident from the phenomena of its ſolution in the nitrous acid, and the revival of its calx in inflammable air. According to the old theory, there is a loſs of ſome part of its phlogiſton in the ſolution of mercury in the nitrous acid, ſince nitrous air is procured in the proceſs. And though it may be revived from its precipitates by mere heat, yet if it be revived in a veſſel of inflammable air, it will imbibe it in great quantities. Mercury revived in theſe circumſtances muſt contain more phlogiſton than that which is revived from

from the ſame calx by mere heat. But though mercury revived by mere heat after a ſolution in nitrous acid muſt have a deficiency of phlogiſton, and when it is revived from precipitate per ſe inflammable air muſt contain a redundancy of the ſame principle, yet there will hardly be a doubt but that, in all chemical proceſſes, it would exhibit the ſame phenomena.

In all other caſes of the calcination of metals in air, which I have called the *phlogiſtication* of the air, it is not only evident that they gain ſomething, which adds to their weight, but that they likewiſe part with ſomething. The moſt ſimple of theſe proceſſes is the expoſing iron to the heat of a burning lens in confined air, in conſequence of which the air is diminiſhed, and the iron becomes a calx. But that there is ſomething emitted from the iron in this proceſs is evident from the ſtrong *ſmell* which ariſes from it. If the proceſs be continued, inflammable air will be produced, if there be any moiſture at hand to form the baſis of it. From this it is at leaſt probable, that, as the proceſs went on in an uniform manner, the ſame ſubſtance, viz. the baſis of inflammable air, was continually iſſuing from it; and this is the ſubſtance, or principle, to which we give the name of *phlogiſton*.

That

That the effect of this proceſs is not, as the antiphlogiſtians aſſert, the mere ſeparation of the dephlogiſticated from the phlogiſticated air in that of the atmoſphere, I have proved in a courſe of experiments, in which I have ſhewn that a conſiderable part of the phlogiſticated air that is found after this proceſs is formed in the courſe of it, by the union of the phlogiſton from the iron with the dephlogiſticated air. And if the calcination of the iron in this proceſs be always attended with the loſs of ſome conſtituent part of it, the ſame is, no doubt, the caſe with all other calcinations of the ſame metal, and alſo thoſe of all other metals. And farther, if the *metals* be compound ſubſtances, containing phlogiſton united to ſome baſe, the ſame is the caſe with *ſulphur* and *phoſphorus*, becauſe they become acids when they are uſed in the ſame proceſs.

According to the antiphlogiſtic theory, the inflammable air that is produced in the ſolution of metals in any acid comes wholly from the water combined with it, and not at all from the metal diſſolved. But the advocates for this theory do not ſeem to have attended to one neceſſary conſequence of this ſuppoſition. According to their own principles, water conſiſts of eighty-ſeven parts of oxygen, to only thirteen of hydrogen, in every hundred, which is nearly ſeven times as much of the former as

of

of the latter. Conſequently, ſince nothing but hydrogen eſcapes in the proceſs, there muſt remain, from this decompoſition of the water, ſeven times as much oxygen in the ſolution. But both Mr. Lavoiſier and Mr. de la Place ſay (*Examination of Mr. Kirwan's Treatiſe*, p. 197, 198), what I doubt not is ſtrictly true, that after the proceſs the acid will ſaturate exactly the ſame quantity (they do not ſay more) of alkali, that it would have done before; whereas, with the addition of ſo much oxygen, it ought to ſaturate conſiderably more. If the oxygen from the decompoſition of the water do not join that in the acid, what becomes of it?

If this caſe be analogous to that of the ſuppoſed decompoſition of water by hot iron, the oxygen ought to be lodged in the iron, and compoſe finery cinder. But this ſubſtance is not ſoluble in vitriolic acid, if that be employed in the experiment; and when it is diſſolved in the marine acid, it does not dephlogiſticate it, as minium, and other ſubſtances containing oxygen, do. It is evident, therefore, that there is no addition of oxygen in this proceſs, conſequently no decompoſition of water in the caſe, and that the inflammable air muſt come from the decompoſition of the iron.

SEC-

SECTION II.

OF THE COMPOSITION AND DECOMPOSITION OF WATER.

THE antiphlogiſtic theory has received its greateſt ſupport from the ſuppoſed diſcovery that water is reſolvable into two principles, one that of *oxygen*, the baſe of dephlogiſticated air, and the other, becauſe it has no other origin than water, *hydrogen*, or that which, with the addition of *calorique*, or the element of *heat*, conſtitutes inflammable air. " One of the " parts of the modern doctrine the moſt ſolidly " eſtabliſhed, ſay Mr. Berthollet, and the other " authors of the *Report* on this ſubject (*Exa-* " *mination of Kirwan*, p. 17), is the formation, " the decompoſition, and recompoſition, of wa- " ter. And how can we doubt of it, when " we ſee that, in burning together fifteen " grains of inflammable air, and eighty-five " of vital air, we obtain exactly an hundred " grains of water, in which, by decompoſition, " we find again the ſame principles, and in " the ſame proportions. If we doubt of a " truth eſtabliſhed by experiments ſo ſimple, " and palpable, there would be nothing cer- " tain in natural philoſophy. We might even " queſtion whether vitriolated tartar be com-

" poſed

" poſed of vitriolic acid and fixed alkali, or ſal
" ammoniac of the marine acid and volatile al-
" kali, &c. &c. For the proofs that we have
" of the compoſition of theſe ſalts are of the
" ſame kind, and not more rigorous, than thoſe
" which eſtabliſh the compoſition of water. No-
" thing, perhaps, more clearly proves the weak-
" neſs of the old theory, than the forced ex-
" planations that have been attempted to be
" given of theſe experiments."

Notwithſtanding the confidence thus ſtrongly expreſſed by theſe able and experienced chemiſts, I muſt take the liberty to ſay, that the experiments to which they allude appear to me to be very liable to exception, and that the doctrine of phlogiſton eaſily accounts for all that they obſerved.

Their proof that water is decompoſed, and reſolved into two kinds of air, is that when ſteam is made to paſs over red-hot iron inflammable air is produced, and the iron acquires an addition of weight, becoming what is called *finery cinder*; but what they call *oxide of iron*, ſuppoſing that there is lodged in it the oxygen which was one of the conſtituent parts of the water expended in the proceſs, while the other part, or the hydrogen, with the addition of heat, aſſumed the form of inflammable air.

But in order to prove that this addition of weight to the iron is really oxygen, they ought to be

be able to exhibit it in the form of dephlogiſticated air, or of ſome other ſubſtance into which oxygen is allowed to enter, and this they have not done. Iron that has really imbibed air, or the common *ruſt of iron*, has a very different appearance from this finery cinder, being *red*, and not *black*; and when treated in ſimilar proceſſes, exhibits very different reſults. Mr. Fourcroy ſays, (Ib. p. 251), that this finery cinder is "iron partially oxygenated." But if that were the caſe, it would go on to attract more oxygen, and in time become a proper ruſt of iron, completely oxygenated. But this is ſo far from being the caſe, that finery cinder never will acquire ruſt; which ſhews that the iron in this ſtate is ſaturated with ſome very different principle, which even excludes that which would have converted it into ruſt.

However, neither this, nor any other calx of iron, can be revived unleſs it be heated in inflammable air, which it eagerly imbibes, or in contact with ſome other ſubſtance which has been ſuppoſed to contain phlogiſton. The probability therefore is, that the phlogiſton then enters this calx of iron, replacing that which had been expelled to form the inflammable air. Nor can any inflammable air be procured in this proceſs with ſteam, but by means of ſome ſubſtance which has been ſuppoſed to contain phlogiſton. Where then, is

the certain proof that water is decompoſed in this proceſs?

It may be ſaid that the oxygen imbibed by this iron, being expelled by heat in contact with inflammable air, unites with that air, and with it conſtitutes the water which is found after the proceſs. But for any thing that appears, this water may be that which the iron had imbibed, and which can only be expelled from it by the entrance of that phlogiſton which it had loſt.

This is the more probable, ſince, when any other ſubſtance which is certainly known to contain oxygen, is heated in the ſame circumſtances, *fixed air* (which is allowed to contain oxygen) is found, and this is not the caſe with this calx of iron. If, for example, precipitate per ſe, or minium, be heated in inflammable air, the mercury and the lead will be revived, and a conſiderable quantity of fixed air will be produced at the ſame time. But if the air be previouſly expelled from the minium, which converts it into a yellow ſubſtance called *maſſicot*, though the lead will be revived, no fixed air will be generated. Since, therefore, the reſult of treating finery cinder and maſſicot is preciſely the ſame, in the ſame circumſtances, we are fully authoriſed to conclude that the ſubſtances themſelves are ſimilar, and conſequently that the

finery

finery cinder contains no more oxygen than maſſicot.

In another important reſpect finery cinder and maſſicot are ſimilar. They are both ſoluble in marine acid without dephlogiſticating it, which minium inſtantly does. And yet Mr. Berthollet ſays, *Annales de Chymie*. Vol. 3. p. 96, that "the heat by which minium becomes "maſſicot cannot change its nature." What is the evidence of a change in the *nature* of any thing, but a change of its *properties?* On the whole, therefore, the probability is, that when iron is converted into finery cinder, it loſes its phlogiſton, and imbibes only water; and that when it is reconverted into iron, it parts with the water, and recovers its phlogiſton. N. B. The experiment with the maſſicot muſt be tried preſently after it is made, ſince it will very ſoon imbibe air from the atmoſphere.

In this place I would obſerve that, if it be admitted that there is a principle in inflammable air, which, being imbibed by the calx of a metal, converts it into a metallic ſubſtance, it will follow that the ſame principle is contained in charcoal, and other combuſtible ſubſtances; becauſe they will all produce the ſame effect, and therefore that the principle of inflammability, or phlogiſton, is the ſame in them all.

Another pretended proof that water is compoſed of dephlogiſticated and inflammable air, is that when the latter is burned ſlowly in the former, they both diſappear, and a quantity of water is produced, equal to their weight. I do not, however, find that it was in more than a ſingle experiment that water ſo produced is ſaid to have been entirely free from acidity, though this experiment was on a large ſcale, not leſs than twelve ounces of water being procured. But the apparatus employed does not appear to me to admit of ſo much accuracy as the concluſion requires; and there is too much of correction, allowance, and computation, in deducing the reſult. Alſo it is, after all, acknowledged that, after decompoſing this quantity of the two kinds of air, and making all the allowance they could for the phlogiſticated air, or *azote*, in the dephlogiſticated air, they found fifty-one cubic inches of this kind of air more than they could well account for. This quantity, therefore, and perhaps ſomething more (ſince the operators were intereſted to make it as ſmall as poſſible) muſt have been formed in the proceſs. And when this kind of air, as well as inflammable, is decompoſed together with dephlogiſticated air, nitrous acid is produced. The probability therefore is, that the acidifying principle, or oxygen, in the dephlogiſticated air which they decompoſed, was contained in that phlo-

phlogiſticated air, and that, had the proceſs been conducted in any other manner, it would have aſſumed the form of nitrous acid. They acknowledge that, except when the inflammable air was burned *in the ſloweſt manner*, the water they produced had more or leſs of acidity.

The experiments which I made on the decompoſition of theſe two kinds of air in *cloſe veſſels*, appear to me to be much leſs liable to exception, and the concluſion drawn from them is the reverſe of that of the French philoſophers.

When dephlogiſticated and inflammable air, in the proportion of a little more than one meaſure of the former to two of the latter, both ſo pure as to contain no ſenſible quantity of phlogiſticated air, are incloſed in a glaſs or copper veſſel, and decompoſed by taking an electric ſpark in it, a highly phlogiſticated nitrous acid is inſtantly produced; and the purer the airs are, the ſtronger is the acid found to be. If phlogiſticated air be purpoſely introduced into this mixture of dephlogiſticated and inflammable air, it is not affected by the proceſs, though, when there is a conſiderable deficiency of inflammable air, the dephlogiſticated air, for want of it, will unite with the phlogiſticated air, and, as in Mr. Cavendiſh's experiment, form the ſame acid. But ſince both the kinds of air, viz. the inflammable and

the phlogiſticated, contribute to form the ſame acid, they muſt contain the ſame principle, viz. phlogiſton.

If there be a redundancy of inflammable air in this proceſs, no acid will be produced, as in the great experiment of the French chemiſts, but in the place of it there will be a quantity of phlogiſticated air. A conſiderable quantity of *water* is always produced in theſe decompoſitions of air. But this circumſtance only proves that the greateſt part of the *weight* of all kinds of air is water. I have, in my experiments on *terra ponderoſa aerata* demonſtrated that water conſtitutes about half the weight of fixed air.

The reaſon, no doubt, why, in the experiments of the French chemiſts, the water they produced was not without acidity, whenever the flame they made uſe of was too ſtrong, was that, in that caſe, more of the dephlogiſticated air in proportion to the inflammable was conſumed, than when the flame was weak; ſo that the reſults of their experiments exactly coincide with thoſe of mine.

When the decompoſition of dephlogiſticated and inflammable air is made in a glaſs veſſel, a peculiarly *denſe vapour* is formed, which the eye can eaſily diſtinguiſh not to be mere vapour of *water*, and if the juice of turnſole be put into the veſſel, it immediately becomes of

a deep red, which ſhews that it was an acid vapour.

Since the acid that I procured in this proceſs was in conſiderable quantity, and no phlogiſticated air was preſent (for in the laſt of my experiments I did not even make uſe of an air-pump, but firſt filled the veſſel with water, and then diſplaced it by the mixture of the airs), I do not ſee how it is poſſible to account for the formation of this acid but from the union of the two kinds of air; and it can hardly be ſuppoſed that, in the very ſame proceſs, the decompoſition of the ſame ſubſtances ſhould compoſe others ſo very different from each other as *water* and *ſpirit of nitre*. I think I have ſufficiently accounted for the reſult of the experiments made by the French chemiſts on the common hypotheſis, which ſuppoſes inflammable air to contain phlogiſton, but I do not yet ſee how it is poſſible for them to explain mine on theirs, according to which there is no ſuch principle in nature. Upon the whole, it does not appear to me that the evidence either for the compoſition, or the decompoſition of water, is at all ſatisfactory; and certainly the arguments in ſupport of an hypotheſis ſo extraordinary, and ſo novel, ought to be of the moſt concluſive kind.

SECTION III.

OTHER OBJECTIONS TO THE ANTIPHLOGISTIC THEORY.

HAVING conſidered the evidence that has been alleged in ſupport of the antiphlogiſtic theory, and found it to be inſufficient, I ſhall, in this ſection, mention a few objections that may be made to it from other conſiderations.

1. If inflammable air, or hydrogen, be nothing more than a component part of water, it could never be produced but in circumſtances in which either water itſelf, or ſomething into which water is known to enter, is preſent. But in my experiments on heating finery cinder together with charcoal, inflammable air is produced, though, according to the new theory, no water is concerned. According to this theory, finery cinder, called the *oxide of iron*, conſiſts of nothing beſides iron and oxygen; and the charcoal, made with the greateſt degree of heat that can be applied, is equally free from water; and yet when theſe two ſubſtances are mixed together, and expoſed to heat, they yield inflammable air in the greateſt abundance.

This fact I cannot account for on the principles of the new theory; but nothing is eaſier on thoſe of the old. For the finery cinder containing

taining water, as one of its component parts, gives it out to any ſubſtance from which it can receive phlogiſton in return. The water, therefore, from the finery cinder uniting with the charcoal makes the inflammable air, at the ſame time that part of the phlogiſton from the charcoal contributes to revive the iron. Inflammable air of the very ſame kind is procured when ſteam is made to paſs over red-hot charcoal,

2. Though the new theory diſcards phlogiſton, and in this reſpect is more ſimple than the old, it admits another new principle, to which its advocates give the name of *carbone*, which they define to be the ſame thing with charcoal, free from earth, ſalts, and all other extraneous ſubſtances; and whereas we ſay that fixed air conſiſts of inflammable air and dephlogiſticated air, or oxygen, they ſay that it conſiſts of this carbone diſſolved in dephlogiſticated air. See *Examination of Mr. Kirwan*, p. 79. Mr. Lavoiſier ſays, Ib. p. 63, that "wherever fixed air has been obtained, there "is charcoal." They therefore call it the *carbonic acid.*

But in many of my experiments large quantities of fixed air have been procured where neither charcoal, nor any thing containing charcoal, was concerned, or none in quantity ſufficient to account for it. When the pureſt malleable

malleable iron is heated in dephlogiſticated air, or in vitriolic acid air, a conſiderable quantity of fixed air is formed. It is ſaid that *plumbago* is contained in iron. But it is not found in malleable iron, and leaſt of all in the *air* that is expelled from it. Fixed air is alſo produced by reviving minium in inflammable air, and if charcoal of copper be heated in dephlogiſticated air, a quantity of fixed air equal to nine-tenths of the dephlogiſticated air will be formed. More than thirty ounce meaſures of the pureſt fixed air were by this means procured from ſix grains of this charcoal, which is made by the union of ſpirit of wine and this metal.

Laſtly, fixed air is procured in great abundance in animal reſpiration. It is true that fixed air is procured by expoſing lime-water to atmoſpherical air, but it is never procured by this means in air confined in any veſſel. There muſt, for this purpoſe, be an open communication with the atmoſphere. But fixed air will be procured in great abundance by breathing air contained in the ſmalleſt receiver, and eſpecially if the air be dephlogiſticated. It muſt therefore be formed by phlogiſton, or ſomething emitted from the lungs, uniting with the dephlogiſticated air which it meets there. It may be ſaid that ſince we feed in a great meaſure upon vegetables (and even animal food is originally

nally formed from them) and this principle of *carbone* is found in all vegetables, *this* may be the ſubſtance that is exhaled from the lungs. But ſince, in this proceſs, it forms the ſame ſubſtance that inflammable air from iron does with dephlogiſticated air, or oxygen, it muſt be the ſame thing with it; and then this *carbone* will only be another name for *phlogiſton*.

3. The antiphlogiſtians always ſuppoſe *azote*, or phlogiſticated air, to be a ſimple ſubſtance, though I think abundant evidence has been given (and more will be found in my laſt memoir, printed in the *Tranſactions of the Philoſophical Society at Philadelphia*),* that it is compoſed of phlogiſton and dephlogiſticated air.

4. As to the *new nomenclature*, adapted to the new theory, no objection would be made to it, if it were formed, as is pretended, upon a knowledge of the real conſtitution of natural ſubſtances; but we cannot adopt one, the principles of which we conceive not to be ſufficiently aſcertained. For other objections to this nomenclature, I refer to the Preface to *Mr. Keir*'s excellent *Dictionary of Chemiſtry*. However, whether we approve of this new language or not, it is now ſo generally adopted, that we are under a neceſſity of learning, though not of uſing it.

On the whole, I cannot help ſaying, that it appears to me not a little extraordinary, that

* See the preceding article.

a theory

a theory ſo new, and of ſuch importance, overturning every thing that was thought to be the beſt eſtabliſhed in chemiſtry, ſhould reſt on ſo very narrow and precarious a foundation, the experiments adduced in ſupport of it being not only ambiguous, or explicable on either hypotheſis, but exceedingly few. I think I have recited them all, and that on which the greateſt ſtreſs is laid, viz. that of the formation of water from the decompoſition of the two kinds of air, has not been ſufficiently repeated. Indeed, it requires ſo difficult and expenſive an apparatus, and ſo many precautions in the uſe of it, that the frequent repetition of the experiment cannot be expected; and in theſe circumſtances the practiced experimenter cannot help ſuſpecting the accuracy of the reſult, and conſequently the certainty of the concluſion.

But I check myſelf. It does not become one of a minority, and eſpecially of ſo ſmall a minority, to ſpeak or write with confidence; and though I have endeavoured to keep my eyes open, and to be as attentive as I could to every thing that has been done in this buſineſs, I may have overlooked ſome circumſtances which have impreſſed the minds of others, and their ſagacity is at leaſt equal to mine.

The phlogiſtic theory is not without its difficulties. The chief of them is that we are not able to aſcertain the *weight* of phlogiſton, or indeed

that of the oxygenous principle. But neither do any of us pretend to have weighed *light*, or the element of *heat*, though we do not doubt but that they are properly *substances*, capable, by their addition, or abstraction, of making great changes in the properties of bodies, and of being transmitted from one substance to another.

N. B. For answers to the objections of Mr. Lavoisier and Mr. Berthollet to some experiments of mine relating to this subject, I refer to the last edition of my Observations on Air, Vol. III. p. 554.

THE END.

Also may be had of J. Johnson,

1. AN ESSAY

ON CHEMICAL NOMENCLATURE,

BY

STEPHEN DICKSON, M. D.

State Physician in Ireland; Professor of the Practice of Medicine in Trinity College, Dublin, &c.

IN WHICH ARE COMPRISED

OBSERVATIONS ON THE SAME SUBJECT,

BY

RICHARD KIRWAN, LL. D. F. R. S. &c.

PRICE FIVE SHILLINGS.

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2. A

TRANSLATION

OF THE

TABLE OF CHEMICAL NOMENCLATURE,

PROPOSED BY

DE GUYTTON, FORMERLY DE MORVEAU,
LAVOISIER, BERTHOLET, AND
DE FOURCROY;

WITH ADDITIONS AND ALTERATIONS:

TO WHICH ARE PREFIXED

AN EXPLANATION OF THE TERMS,

AND SOME

OBSERVATIONS

ON THE

NEW SYSTEM OF CHEMISTRY.

By G. PEARSON, M. D.

PRICE SIX SHILLINGS.